dimensions. In that case there is no necessity to employ sliding prisms and scale, or equivalent, as the part of the field where the coincidence occurs depends upon the distance of the object, and thus a scale of distance at the focus of the eyepiece is all that is needed. Of all methods of using the angle of parallax to find the distance, the most attractive is one proposed by a workman in the Zeiss works, and which, after much difficulty in its elaboration had been overcome, was shown to the present writer by the late Dr. Czapski at the Paris Exhibition of 1900. In this instrument the right and left beams are received by the right and left eyes respectively of the observer, and owing to the distance between the two beams entering the instrument a superstereoscopic view of the object is seen. At the same time each eye sees in the field of view a scale of distance, but the two scales are differently ruled in such manner that the eyes combine them stereoscopically and the scale of distance appears projected away into space. It was fascinating to sweep this scale past more or less distant buildings and see the divisions of the distance scale pass behind or in front of the different objects, or to look up the Eiffel Tower and tickle the members of the framework with the nearer divisions. For the purpose of aircraft range-finding this method, on account of its speed, would appear to have great advantages, and even if it does not equal in accuracy the more deliberate methods of other range-finders, this cannot be of consequence when the range is changing at so high a rate. Some discussion of this type of range-finder by Prof. Cheshire would have been very valuable. The number of the German patent is 82,571, and the date July, 1895. A description is to be found in the second volume of the collected papers of Ernst Abbe, published by Gustav Fischer in the year 1906.

Returning now to the question of the limitation of accuracy, the figures quoted as having been obtained on a Barr and Stroud instrument are important and surprising. The base of the instrument was three yards, but the diameter of the object glasses is not stated. Using an optically prepared artificial object, the accuracy of setting obtained by an experienced and highly skilled observer was such that the mean error was about one-fifth of a second of arc, *i.e.* an angle with a circular measure of one divided by a million. When it is remembered that the defining power of a telescope as measured by the diameter of the star image is about 4·5 seconds of arc divided by the aperture in inches, this is equivalent to saying that the aligning power of this range-finder is equal to the separating power of a perfect telescope of about 22-in. aperture, and that irrespective of the length of its base. Or if, as is likely, the aperture is about 2 in., the aligning power is more than ten times the possible separating power. Similarly, on multiplying by the magnifying power, it appears that the aligning power of the unaided eye is in the neighbourhood of 3 seconds of arc, which is still more surprising when it is remembered that the separating power is certainly insufficient to divide 60 seconds. It would be interesting to ascertain what is the aiming power of a good billiard player when, for instance, the object ball is near the striking ball and far from the pocket, but when, nevertheless, with this coefficient against him, he can time after time drive the ball clean into the pocket. That, whatever it is, must be very great, but it must be exceeded by the aligning power of the eye in the comfortable use of a good range-finder. Figures such as are here given must be realised before the skill and marvellous attainment of the designer and constructor of the modern range-finder can be appreciated. There is much more in this pamphlet that it would be interesting to follow if space were available.

C. V. BOYS.

THE RELATIONS OF MATHEMATICS TO THE NATURAL SCIENCES.

BY a happy coincidence, the addresses of the retiring presidents of two leading mathematical societies, delivered almost simultaneously, follow similar lines, although from somewhat different angles of view, and are of unusual interest for the man of science whose surmises regarding natural phenomena receive their ultimate justification from mathematical reasoning. Such a man has had cause more and more in recent years to deplore the divorce between the more striking mathematical developments of the present time and those which are urgently necessary as an inspiration to progress in his own work. For, as the two presidents point out, the insistent call for help to the pure mathematician has now begun, though perhaps reluctantly, to take shape even from the biological sciences.

Prof. E. W. Brown, in his address to the twenty-third annual meeting of the American Mathematical Society, selected the subject the title of which we have borrowed, and indicated somewhat precisely the types of work really needed from the pure mathematician in this regard, and their capacity for furnishing a fruitful field of research of great interest to any willing investigator. Sir Joseph Larmor, in his address to the London Mathematical Society in November, 1916, limited his detailed remarks more especially to the scope and limitations of the harmonic analysis associated with the name of Fourier. The problems connected with periodic phenomena were evidently predominant also in the mind of Prof. Brown during the preparation of his address, and the necessity for a Fourier type of treatment of such problems renders the two addresses complementary in the regions in which they are not closely parallel.

We may turn, in the first place, to the more general point of view present in both addresses, and outlined in greater detail in Prof. Brown's. Pure mathematics is a science or an art which is self-contained, and requires for its development no external inspiration. Applied mathematics is an aid towards the development of the natural sciences, and in fact of all investigations which depend on deduction from exact statements. Such statements are, of course, founded not on axioms, but on physical laws which sum up the results of series of experiments, and these laws no longer, as in the past, serve to suggest suitable axioms and profitable lines of development of pure mathematics as an art. So large a body of doctrine, in fact, has pure mathematics become that isolation is marked among its many branches, and one mind can no longer be fully conversant with each of them. The task of our presidents, in attempting a fusion between pure and applied mathematics, becomes more and more difficult.

Prof. Brown points out one fundamental difficulty in the lack of standardisation of mathematical symbols. In spite of the fixed character of the underlying principles, such a symbol as (1) may still denote a number, operator, group, function, axiom, or convention, and any of these may have special limitations for the purpose in view. He suggests that the task of a reader of several members should be facilitated by extending the principle now used in the case of the special type adopted to represent vectors. Such a prearranged system would have special advantages in the subsequent compilation of any future mathematical encyclopædia. Prof. Brown pleads also for an extension of the growing practice, even at the cost of artistic appearance, of printing a summary at the end of each published paper.

These and other purely mechanical aids to the student of science are, of course, only side-issues, and do not touch the main problem of evoking, on the part of the pure mathematician, an interest in the applications. The pure mathematician has not the leisure necessary for familiarity with the history and essentials of a proposed problem, but he could assist by turning the thoughts of his better students into such a direction. When he does become interested in an application, he usually studies only the mathematical methods tried more crudely by others. His interest, in fact, lies more in the logic of the matter than in any co-ordination of new phenomena which may be obtained. Yet at the same time he must not be blamed, for the physicist and engineer rarely present their problems in such a form that the mathematician can even begin to seek a solution. He does not know what approximations he may make and yet retain a solution of value. The proper function of a treatise on applied mathematics is to give strict formulations of problems and an account also of the principles which underlie good physical approximations. The applied mathematician who can fulfil this function, and intervene between the mathematician and the experimenter, is now lamentably rare. The temptation to go to one of the extremes is too strong under the present system, though Prof. Brown suggests various ways in which such men could be encouraged to steer the middle course.

The fundamental subjects which, from the present point of view, demand systematic examination, and, more especially, simple exposition from the mathematician, are: the numerical solution of classes of differential equations, symbolic forms adapted for rapid numerical calculation, reduction of a series of numbers to the best formula, and Fourier and other representations of periodic phenomena. Under this last heading a considerable contribution is made by Sir Joseph Larmor's address, which cannot in this respect be noticed at all adequately in our present space. But it is readily accessible, and this fact somewhat precludes the necessity. In so far as it is general the views expressed are essentially similar to those outlined above, and it includes, moreover, an instructive account of the history and present state of the society, with suggestions towards its future adaptation to changing conditions.

In his critical analysis of the Fourier harmonic method Sir Joseph sketches the history of its development, and afterwards points to an insistent question: What is to be done with the accumulated observational data such as are being piled up by meteorologists and statisticians, and to what extent should they be continued? Such questions are of the essence of pure mathematics and not strictly of its technical application. It is a curious fact that progress in such directions was practically stopped by difficulties in running the Kelvin integrating machine. Sir Joseph Larmor makes a powerful appeal to the pure mathematician to revive his former interest in such problems, and cites the work of Schuster as a striking illustration of the success which could be obtained by an organised attack. We may cite, as another illustration, Sir Joseph's own discussion of some of the problems of radiation, which forms the remainder of his address, for it presents many sides of the question which have been only too imperfectly considered by those who work with any aspect of the Fourier analysis.

We can only repeat that it is a fortunate event, and perhaps a sign of the times, that the presidents of the two leading mathematical societies in the English-speaking world should have chosen the same ground so closely, and independently expressed concordant opinions even in points of detail. This fact must surely stimulate workers to an interest in these questions, the elucidation of which, even if only partial, would be a fundamental gain to the whole range of work in the province of natural science.

J. W. NICHOLSON.

PRECISE LEVELLING IN THE WEST OF ENGLAND.[1]

THIS recently published Professional Paper of the Ordnance Survey gives an interesting account of the revision of a line of precise levelling which had been carried out under the direction of a committee of the British Association in the years 1837 and 1838. The line was run from Axmouth, on the coast of the English Channel, to three points on the southern coast of the Bristol Channel, and the terminal points were marked with metal bolts "to afford a basis for a comparison with the position of the lines then determined, at present, and at any future period." When the revision of the primary levelling network of Great Britain was undertaken the revision of this particular line was included in order to see whether there was any indication of earth movement, and in the course of the last three years it has been found practicable to carry out this work by the reserve levelling staff which has to be maintained at Southampton. The earlier levelling was carried out by Mr. T. G. Bunt, and full details are given by Dr. W. Whewell and him in the report of the British Association for 1838.

He used a level by Simms which had a telescope 14 in. in length and a magnification of 26. The bubble is said to have been affected by a movement of 1/100,000 in. of either end. The staff used was at first of brass, but this being found unsatisfactory, it was replaced by one of seasoned oak 9 ft. long and having scales on both sides. Nothing is stated about the verification of the staff divisions. The staff was read with the aid of a vane or target, of which the position was read by a vernier to 1/500 ft., and it is stated that the average error of a single reading was 1/250 in. Lines were levelled in both directions from beginning to end, then from end to beginning, and the discrepancies found are recorded. Mr. Bunt mentions a systematic error which he experienced, viz. that "the heights of all points came out less by the levels returning than by the levels going," and from Portishead to Axmouth, a distance of seventy-four miles, the discrepancy between forward and backward levelling was 1·029 ft. The old levelling books are not now to be found, so that the comparison with modern work could only be made over the distances between Axmouth, Axmouth Church, Stolford, and Perry Farm, where the old marks are still existing.

The discrepancy between the older and the new levelling from Axmouth to Perry Farm, a distance of fifty-seven miles, is but 0·92 in., though at Stolford, fifty-five miles, it reached 2·11 in. The amount of the accidental and systematic errors of Bunt's levelling computed by the formulæ adopted by the International Geodetic Commission is 1·0 mm. and 0·9 mm. per kilometre respectively, against the limits of 1 mm. and 0·2 mm. per kilometre, as laid down by international agreement for precise levelling.

The conclusion arrived at is that there is no evidence of any change in the relative levels of the marks near the shores of the English Channel and the Bristol Channel.

The Ordnance Survey levelling was executed with a Zeiss No. 3 pattern 14-in. level with a parallel plate object-glass micrometer, and invar levelling staves. The operation is one of much interest as affording a comparison between the best class of levelling work in this country at the two periods.

H. G. L.

[1] Report on the Re-levelling in 1915–17 of a Line from the English Channel to the Bristol Channel. Ordnance Survey Professional Papers. New Series, No. 4, 1917. Price 6*d*.